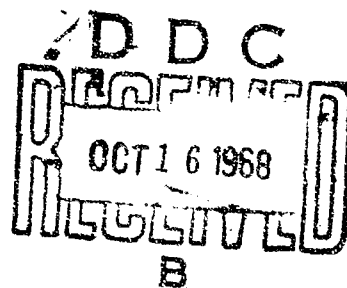


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PLANNING PHENOMENA-ORIENTED RESEARCH IN
A MISSION-ORIENTED ORGANIZATION



Twelfth Institute on Research Administration
Center for Technology and Administration
The American University

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59

PLANNING PHENOMENA-ORIENTED RESEARCH IN
A MISSION-ORIENTED ORGANIZATION

A selection of papers presented at the Twelfth
Institute on Research Administration held
24-27 April, 1967, Washington, D.C. under the
sponsorship of The American University

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INTRODUCTION

This volume represents presentations and associated remarks and commentary on scientific research planning from an organizational standpoint. They were presented at Session II of The Institute on Management Technology and the Optimization of Research and Development, the 12th Institute on Research Administration, sponsored by The American University, Institute on Research Administration, 24-27 April 1967, Washington, D.C. The session was entitled "Planning Phenomena-oriented Research in a Mission-oriented Organization." The authors considered research planning processes in The Boeing Company, Bell Telephone Laboratories, NASA and The Air Force Office of Scientific Research (OAR). Various aspects considered included research goals and products, and strategy of operation; choice of program areas and projects; staff recruitment and career development; relationships to corporate and organizational structure; decision making processes; research communication; and motivation of scientists and engineers.

Contents

	<u>Page</u>
I INTRODUCTORY REMARKS	1
Howard M. Vollmer Manager, Organization and Manpower Studies Technology Management Programs Stanford Research Institute	
II PLANNING PHENOMENA-ORIENTED RESEARCH IN THE BOEING COMPANY ...	4
Guilford L. Hollinsworth Director, Scientific Research Laboratories The Boeing Company	
III PLANNING PHENOMENA-ORIENTED RESEARCH IN THE BELL TELEPHONE LABORATORIES	11
John K. Galt Director, Solid State Electronics Research Laboratory Bell Telephone Laboratories	
IV PLANNING PHENOMENA-ORIENTED RESEARCH IN NASA	21
Lester C. Van Atta Assistant Director, Electromagnetic Research National Aeronautics and Space Administration	
V PLANNING PHENOMENA-ORIENTED RESEARCH IN AFOSR	30
William J. Price Exec Director, Air Force Office of Scientific Research Office of Aerospace Research United States Air Force	
VI SOME FINDINGS FROM STUDIES OF SCIENTISTS IN ORGANIZATIONS	46
Donald C. Pelz Professor of Psychology and Program Director Institute for Social Research, Survey Research Center University of Michigan	

INTRODUCTORY REMARKS

by

Howard M. Vollmer

Manager, Organization and Manpower Studies

Technology Management Programs

Stanford Research Institute

Two years ago, at the American University Tenth Institute on Research Administration, we had a panel of speakers from industry, government, and universities on a topic that provided a good background for our topic this afternoon. In that session, the speakers discussed the reasons why some industrial corporations and certain government agencies find it valuable to have a capability to understand and to conduct fundamental research in their organizations. They also described how a fundamental research organization in industry or government is typically organized so that good research is accomplished and so that useful outputs from this research get applied in engineering design, product development, manufacturing, and other operating functions in their parent organizations. Moreover, they discussed some of the personnel problems connected with the staffing of such organizations with high caliber scientists and other technical professional personnel. A transcript of this discussion is available under the title, The Fundamental Research Activity in a Technology-Dependent Organization, from the Clearinghouse for Federal Scientific and Technological Information, Springfield, Virginia, 22151. AD 628 747

Our object today is to move out somewhat beyond the topic of two years ago. Having established that the kind of research we are talking about today is indeed valuable and useful in certain kinds of larger organizational contexts, and having established that this kind of research can be properly organized and staffed, we want to discuss a future-oriented dimension. Specifically, what do we know about planning this kind of research?

Our topic has the somewhat ponderous title of "Planning Phenomena-Oriented Research in a Mission-Oriented Organization". We should recognize that this title was very carefully worded by Dr. Price -- the organizer of this session this afternoon, as well as the previous session that I have just mentioned. Perhaps it would be well in this introduction if I spent just a minute talking about what I understand to be the meaning of each one or the terms in this title. No doubt the speakers that follow will make these terms come to life as they describe what they mean in their own particular organizational contexts.

But let's consider the term "mission-oriented organization" here first. In one sense, I suppose that all organizations have

some kind of a mission or goal toward which the overall activities of the organization are supposed to be oriented. Some have more than one mission or goal. Sometimes these missions or goals can be somewhat contradictory. And we know that some organizations seem to be more mission-oriented than others -- that is to say, their members experience more pressure to produce something, or to get something accomplished collectively, than is the case in other organizations. Here this afternoon we want to talk about organizations in which research is conducted, but organizations with a basic overall mission that is something else besides the conduct of research. Their overall mission may be the production and sales of some kind of industrial product or line of products, the provision of a public service, the maintenance of the national defense, the exploration and development of new resources, or some other class of aims. They must do research to accomplish these overall missions. But they are not primarily in the research business. Doing research per se is not part of their overall organizational missions. This basic fact poses certain problems for the planning of research activities that will come out in the following talks.

Now let's turn to the term "phenomena-oriented research." This seems to be the most satisfactory way to label a kind of research that is meant to dig deeply into the nature of some kind of natural phenomena in the world around us -- phenomena that we need to know more about in order to solve some of the problems that many industrial and governmental organizations have today in accomplishing their overall missions successfully. This is not quite the same thing as "basic research" that we talk about when we make the common distinction between basic and applied research in the terms suggested by the National Science Foundation, for example. Much basic research appears to be phenomena-oriented, but so is some applied research -- that is research initiated in response to some practical problem -- if this research also requires the scientist to dig deeply into the fundamental nature of some natural phenomenon. In any case, this kind of research requires special considerations in trying to plan it, as we will see later.

Finally, just a word about "planning." Since research -- and particularly phenomena-oriented research -- is always somewhat a venture into the unknown, we appear to be dealing almost with a contradiction in terms. Some people might feel that we are talking about "planning the unplannable." And yet, we shall see that this is not quite so in the experience of our speakers today. They will point out ways in which one can indeed plan certain matters. Their discussion may consider the extent to which one can plan:

- (1) The goals, products, and strategy of operation of a research organization
- (2) The choice of research program areas
- (3) The choice of research projects to undertake
- (4) The recruitment of scientific staff and their career development
- (5) The organization of research in relation to other activities in the larger organization
- (6) The levels at which key decisions are made with regard to research activities
- (7) The communication of research findings to appropriate users of these findings in the larger organization, and
- (8) The motivation of scientists and the effective use of monetary and nonmonetary incentives to achieve desired goals in an overall matrix of planning activities.

Each of the following speakers will probably touch upon several, if not all, of the above matters in describing phenomena-oriented research planning activities in organizational contexts with which he is acquainted. We hope that you in the audience will be stimulated to respond critically to these comments in the discussion period following these talks, so that we can all be enlightened on how these principles of planning may apply, or may not apply, in your own organizational contexts.

PLANNING PHENOMENA-ORIENTED
RESEARCH IN THE BOEING COMPANY

by

Guilford L. Hollingsworth
Director, Boeing Scientific Research Laboratories
The Boeing Company

What I am going to talk about this afternoon are practical considerations to be made in planning phenomena-oriented research; not the answers. I think, as was pointed out this morning by Ralph Cole, the answers are probably quite different when you consider the specific context in which the research is done.

In the kind of research we are discussing here, the crucial item is people. And research people are just people. They do have somewhat different motivations and different drives, but not so much that there is a great deal of difference in your relationships with them. You must not forget -- the important thing is that you are dealing with people first, and scientists last!

In our organization, as in many others, some of the research management areas we thought were going to be easy later turned out to be hard. Let's see if we can identify some of these significant areas. That is to say, in a problemsolving sense. Let me point out that we are not ordinarily searching for a new product. In our company that is not the mission of basic research. We are pretty much a general problemsolving and obstacle-busting operation. Each of you will have to try to find out how to relate my conclusions to your own organization.

The second major concern is that you are going to have to look at a research area closely, having chosen one. You will, for example, have to look at the activity in the scientific community in this particular area. Let's take the solid state physics. Some very vigorous and vital things are going on in every significant nation of the world in solid state physics.

On the other hand, you may pick an area in which very little is going on. The effect of this factor depends to some degree on what you are trying to accomplish. If you are exploring for new products, perhaps it is a good idea to work where others aren't. That could be a useful way to choose your phenomena-oriented research. The chances are that you will find something new by working in new areas, and this may be better than trampling in the areas where others were.

The Bell Laboratories, you will recall, almost singlehandedly through their work in solid state physics created the transistor industry, and they did a very good job of getting many other people working on it so that the discoveries which they made would begin to cross-fertilize their own research. In this case they were not trying to keep as secrets their ideas, but instead were trying to get them widely known.

So the consideration -- "where is the activity and what is my relation to it going to be" -- should be taken into account. What you do depends somewhat critically on whether you view yourself as a problemsolving organization, an obstacle-buster, or a new product finder, and each of you will have to make your own decisions on this.

Topics for phenomena-oriented research may also depend on whether you view the effort to be cooperative or competitive. In our own case we have just made a decision about choosing a particular research area because we realized that the University of Washington has a somewhat parallel effort going on. In our own judgment, with two parallel efforts going on in the same community, there would be created the important aspect of competition. The scientists will naturally talk to each other, will be writing papers about the same thing, and they will tend to keep each other on their toes. In a situation which is non-competitive, they would not be worried about such things as publishing their ideas first. In that case, some special attention should be given to evaluating the vigor of the effort. There might be some advantage in entering an area where others are working just to have the tools. There are requirements for special instrumentation which could take a vast amount of your working manpower to develop if nobody else is working in the area.

Here I am talking as if you were starting research laboratories right from scratch. This is because it is a simple way to think about it. But these considerations would also apply in much the same sense if you were adding one new element on to your present laboratory.

Certainly you are going to have to think about acquiring your staff when you talk about new research areas to enter. You have to be concerned with whether the people you will need are trained at the universities. Are other companies working in the field? At one time if you wanted to work in linear programs, it would not have done you any good to go to the universities for people. Nobody was teaching linear programming to anybody. You will want to take that into account. Otherwise, you may have to steal scientists from your competitors, or something else like that in order to get your staff.

Another important possibility is to determine if there is anyone on your internal staff that you can deflect in the direction you have chosen. Perhaps there is somebody who is going a little stale in his present work and really has the background for the new work. That's a real victory -- to turn some non-productive person into a very productive one! This is a consideration you might well take into account if you choose to go into a new area, because one of your critical limitations will always be good people -- how to get them. You can't always steal them from your competition. You can't always get them from the universities. You will be in dire straits to make any progress unless you have them.

However, there is a method of solving such a problem on a temporary basis. As a means of exploring a brand new area, or to clarify your ideas as to why you want to go into it, or as a means of supplementing your rather limited team, you can use both contracts and outside consultants. This is often very effective. We often have people come and spend a whole summer helping extend our understanding of some new research area. We may decide then not to go into it, or we may decide we need a permanent staff member for it. You need to be imaginative about this business of probing to see how to get started, and how important this area is going to become. Bringing in experts is often the best way.

In non-competitive areas, this is easy. In competitive areas, it is a little harder because you have to tell all your secrets, or even an expert cannot help you. And it would be unfortunate if you did that and didn't get some productive results for the effort.

When you get around to considering the staff, as individuals, all anyone can say sounds trite. You need to go out and get the very best fellow you can possibly get. Salary should not be a prime consideration. If money is "the" factor to him, he is probably not the man you want. If that is his objective, he hasn't got your objectives in mind.

On the other hand, you must be competitive, and you had better not content yourself with a second-best staff member when you are starting an organization. If you really get someone who is good, real economies will arise from the fact that other good people will now wish to come and work with him. Really first-rate people will come without arguing much about their salaries and other conditions, because they want the chance to work with recognized authorities. So if you can get a start like that -- you should be so lucky -- you have made the first important step.

There are other things you must consider in choosing your staff. There are people on the program and in the audience that are particularly concerned about how to evaluate these people as individuals. Is he a really good guy? Evaluating his training -- well, that's straightforward. His productivity is much harder to evaluate. Various means have been suggested for that, but I hope that you people will dig into this problem and provide some actual quantitative measures of productivity. Sher and Garfield of the Institute for Scientific Information and Howard M. Vollmer of Stanford Research Institute have both made some good studies on this, and you can look into those and see how you agree.* One measure mentioned in both works has been a Science Citation Index -- how many times has the work been cited, not just how many papers did he write? It is very easy to run up an impressive score of papers; it is more difficult to run up an impressive score of citations. I have discovered that this can be done by super-manipulation, but I won't tell you that secret! Maybe we can keep the citation index semi-honest, for it is published, and is useful for some kinds of evaluations.** You must, somehow, make this evaluation -- "Do I have a creative productive guy?" All I can do is give you good wishes -- there is no easy way.

And then will your researchers be motivated to do the work that you want them to do? We've got a scheme that we use in our own laboratory, but this may not be available to all of you. When we get into fairly late-in-the-day negotiations with a scientist, we say: "Why don't you put down in a research proposal what you think would be the most important feature of the work you plan to do if you were to join us? Don't worry about whether we are interested in it or not. If you were going to come here, and you had the right to choose anything you wanted to work on, what would it be, and what would you do about it?" If he can write a pretty imaginative proposal and we can agree that it is something we want done, we have come a long way towards a productive relationship.

* I. H. Sher and E. Garfield, "New Tools for Improving and Evaluating the Effectiveness of Research," and H. M. Vollmer, "Evaluating Two Aspects of Quality in Research Program Effectiveness," both included in M. C. Yovits, et. al., Research Program Effectiveness (New York: Gordon and Breach, 1966.)

** Science Citation Index, published by the Institute for Scientific Information, Philadelphia, Pennsylvania.

Again, in terms of acquiring a staff, you must communicate to scientists outside the organization just what your emphasis is. For example, if you effectively communicate your interest in, say linear programming to those working in that field, the right kind of men will find a way to advertise themselves to you.

One thing that we need to say very little about is the choice of projects. If you have chosen your staff well, there isn't any better way of choosing projects than asking your staff for their recommendations. I think that Kenneth Mees who used to run the Kodak Laboratories said it about as well as anybody. He said: "If a good researcher makes a decision, he is right more than half the time -- that's why he's a good researcher. If his boss makes the decision, he is right about ten percent of the time because he really isn't up-to-date on the subject. If a laboratory director (like me) makes a decision, I might be right one percent of the time because I haven't read any of the recent papers. If a committee of vice-presidents decides, then it is invariably wrong!"

So project selection should be done at the level of the individual, and all you need do to make sure he does it well is to put him in the right environment. You can be sure that with his expert view -- his linear programmers notion of what the world looks like, or whatever he is -- solid state physicist or mathematician -- he looks at all your problems from his own biased viewpoint and says: "I see what I should do about that." And if he is properly motivated, he will. Then you've got to get behind him and provide some support.

Also, you've got to worry about such things as: what about the consequences of failure? Will you be able to ease yourself around it and not worry about it? I believe it is White at Standard Oil who says that one of the important contributions of the basic research type of laboratory organization is that it makes failure respectable. Undoubtedly, if you do not fail a few times, you are not trying very hard. After all, if you are training for the high jump and you always go over the bar, you never know how high you can jump unless you knock it off a few times. So you could say that you had better be alert to the consequences of failure. Undue criticism of failure will guarantee mediocrity.

On the other hand, one of the most tragic things that occurs in the choice of projects is that you don't know what to do with success. You really ought to look down the road and see what should happen if you succeed, and what you are going to do about it. We have a couple of these successes in our own laboratory, and I should blush, because I don't know what to do with them. If you have a success, will it affect an important project, or

will it have no influence that you can see directly? We sometimes find that we have to quietly abandon a successful project and go on to something else because we did not plan what to do if we succeeded. We are too busy worrying about how to succeed. This is worth thinking about. The ability to capitalize on success ought to be one of the factors that you really look at when you are choosing a project, as well as the ability to absorb a failure.

The final consideration in planning a research project is whether you can really support it to the end. After all, if the man will need a cyclotron three steps down the road, you better be prepared to buy it, or not take those first two steps. You ought to think that far ahead, and that is not a trivial consideration.

This is all I care to say now. We can explore the subject further in our panel discussion.

Questions and Answers following Dr. Hollingsworth's Talk

Vollmer

Well, I think we might be open to a few questions at this point. Could I start this questioning myself?

If I understand you rightly, Dr. Hollingsworth, the main factor in planning, as you have brought it out, is getting the right people. If you had to narrow it down to one point, it would be getting the right people. Is that true?

Hollingsworth

I would like to say, once you have chosen the right area, say in the physical sciences or mathematics, then getting the right people is the thing you need to do.

Vollmer

All right. Could I follow through with a more specific question? Is the right kind of person for your laboratory -- the Scientific Laboratory of Boeing which is in the corporate headquarters -- a different kind of person than, say, the kind of research people that they get in other divisions, like the Aerospace Division and others where they do research?

Hollingsworth

I don't think so, Howard. Certainly you can speculate about whether the man who really does a topnotch job in research is the same sort of person as a topnotch man who solves technical problems. I think that among my scientist friends and myself, we have decided -- yes, he is. A really first-rate man finds nothing belittling about solving a practical problem, he is damn good at it. A second-rate guy is a little bit insecure and never feels it is quite appropriate for him to work on something like why one metal sticks to another. He wants only to describe his work in very esoteric and abstract terms, so that you can't really challenge him about whether he knows anything about the subject.

PLANNING PHENOMENA-ORIENTED RESEARCH
IN THE BELL TELEPHONE LABORATORIES

by

John K. Galt

Director, Solid State Electronics Research Laboratory
Bell Telephone Laboratories

I shall, as will others on the panel, address certain topics which arise in connection with the problem stated in the title from the point of view of a specific institution. I trust that this will not turn out to be simply a repetition of the remarks of others on the program, but on the other hand it is perhaps a function of today's discussion to seek consensus in these matters, so the areas of agreement are in some sense what we wish to discover and enlarge upon.

First, a few words about my own institutional point of view. Bell Telephone Laboratories has the mission to discover and develop communications technology for the Bell System. It is a large organization, most of which is not concerned with phenomena-oriented research, but rather with this complex and sophisticated technological mission. For good reasons, which are not the subject of today's discussion, it has undertaken to generate and support some phenomena-oriented research. This support, provided over a substantial period of time, has provided the stable base which is one of the primary needs of such an effort.

This leads to the first topic which I would like to discuss, namely the goals of the organization as a whole. It is of primary importance that such goals be set, first and in the broadest sense by statements of the organization's mission. This is a point at which the phenomena-oriented research area (which I shall henceforth call the research area for short) makes contact with top management, for such goals must ultimately be stated at that level. The importance of such statements can be recognized by noting that the research area bears a primary responsibility for the future of the mission-oriented organization. It is clear that an indispensable tool for meeting this responsibility is an understanding on the part of research management of the broad goals of the mission-oriented organization as a whole. It is appropriate for research management to influence such goals. But it is vital to the development of a viable strategy of research management that, whatever they are, they be understood by both management groups. Furthermore, these goals cannot be purely research goals in an organization in which the research component is a relatively small part, as it surely must be if the organization is mission-oriented.

Given a statement of organizations' goals, a discussion of choices in research management, the second and third topics, becomes easier. Such goals provide research management with a criterion of relevance by means of which to choose disciplines and fields within disciplines in which research is to be done. This is not the only tool needed in order to make such choices wisely, but it is vital, especially in deciding which areas not to work in. Another such tool is an awareness of the location of the frontier of knowledge in the fields which are considered, together with some idea of how hard (or expensive) it is to make progress in them. Still another is the quality of the personnel which can be recruited in the different fields. This is separate from the actual recruiting process, which will be discussed below.

There is another type of choice to be made in managing phenomena-oriented research, which is the third topic in our discussion. That is the choice of research project within the fields chosen as discussed above, and in general after the recruitment of personnel. Here, in my opinion, is an area in which management must tread warily. Research personnel do best the things they themselves choose, and having established the framework discussed above, management is wise to leave a large part of the choice of work projects to the working level scientist. It may at this point be wise to emphasize the value of encouraging working level contacts with personnel in the non-research areas of the organization. Furthermore, some discussion with the working level scientist of the projects on which he proposes to work is surely essential, in order to give him a sounding board for his own thinking, and to avoid errors of judgment such as involvement in a project which clearly requires resources beyond those which can be placed at his disposal. But within such limitations as these, the freedom of the working level scientist to choose his project is a thing to maximize. It may be necessary from time to time to encourage a scientist to seek collaboration with a senior colleague in order to improve his output. But this is not an activity to indulge in freely. The working level scientist in general wants to succeed more strongly even than his management wants him to, and he will usually take the necessary measures on his own. Concerning actual research projects, the most important (and sometimes emotionally demanding) activity for the manager is to seek and use opportunities to express honest enthusiasm for good work accomplished. In an organization where the primary output is ideas which are as yet unknown to the rest of the society, this applause is psychologically vital.

The fourth topic for discussion is recruitment of staff. In an organization of the sort under discussion here, this means the recruitment of people from university graduate schools. Successful selection involves consideration of several relevant professional factors. The first of these is the level of competence of candidates in the technical

fields in which they work. This can be judged in a general way in interviews with the candidate, but a satisfactory evaluation requires contact with the graduate school faculty with whom he has worked. It is this recruiting problem which ultimately establishes most clearly the need for personal professional contact with university graduate school faculty. The activities of a phenomena-oriented research organization are sufficiently similar to those in a university graduate school that the opinions of university faculty are directly relevant, and their contact with candidates is so much more extensive than any which can be established in an employment interview that it is a much more satisfactory base for judgments. There are other important characteristics which can be examined in an employment interview, however. One is simple intellectual vigor; another is flexibility in the sense of interest in more than one field of activity, if only within one discipline. The latter is one aspect of the professional flexibility which sooner or later is needed in any professional career. Finally, and perhaps the point which the interviewer must establish most clearly, a satisfactory candidate must have an interest in a field which the management of the research organization feels is relevant, if only broadly from the point of view of the overall organization. So much for the criteria and methods of recruiting. Personnel considerations, however, also involve the motivations and incentives provided after personnel are recruited. These matters will be discussed later as a separate topic.

Fifth, some organizational questions. Generalizations are not everywhere valid, and it is important to emphasize that personal efforts rather than organizational form lie at the heart of research achievement. Nevertheless, a discipline-oriented organization has certain advantages in the organization under discussion. Perhaps the most important advantage is the improvement in communication this form of organization provides with the university, which is very much discipline-oriented. Such an advantage in communication is of real value in recruiting people with the qualifications to do good research. It may also ease the psychological adjustment of new employees. At the same time, the industrial research organization should not allow the isolation of one department from another. Specific jobs gain immensely from temporary collaboration across disciplinary lines in many cases, and it is important (and also quite possible) to encourage this in spite of the fact that such collaborations cross organizational lines too. But such collaboration is in general based on a transient overlap of interest, and a given man will find it useful in many cases to move from collaboration with one man to later collaboration with another. It is hard to find in this pattern a vital need in the research organization for the project-type of organization which is quite essential in carrying out a purely technological mission.

Another organizational need is a set of mechanisms for liaison between the research area and the rest of the organization. Some of this (perhaps the most important part) is provided by the collaboration across organizational lines mentioned above. In addition, however, it is important for management of the research area to communicate problems and achievements both vertically to their superiors and horizontally to the management of other parts of the mission-oriented organization. The latter function basically requires personal contact across organizational lines by management personnel. The first can be accomplished by staff meetings with several levels of research management present. Initiative in carrying out the horizontal communication function is one of the more important ways of relating the discipline-oriented research organization to the project-oriented technological part of the organization; a search for an overlap in interest is part of this activity. These remarks indicate the value of physical proximity between the research personnel and some at least of the technologically-oriented parts of the organization. It is best if organizational divisions can be made to occur within a physical location.

A sixth topic for discussion is communication patterns. The communication needed within the organization is part of this topic but since it is also relevant to organizational questions, it has been discussed above under that topic. Another aspect of the subject which will be mentioned here is communication to the scientific and technical community. Information passes from the research organization to this community formally by means of published papers and to a lesser extent through patents, but also, I'm sure we all agree, through attendance at technical society meetings and private visits. Such communication is not only important to establish personnel in the professional community. It is an important method by means of which the scientific community brings into the organization up-to-date information on the latest scientific advances. It is, in short, a two-way channel, and the personal contacts involved are an important supplement to reading in the literature about such matters.

The last topic for discussion is career development. If done well, this aids in recruiting; furthermore, all the topics discussed above bear on it. For example, publication is a method of communication, as discussed above. In addition, it is a means by which, as we all know, the young professional man can be assured of finding, in some sense independently of the organization or anyone else, his place in the scientific and technical community. This motivation to do first-class work is probably the strongest one research personnel feel. Publication by members of the organization, and reading of the technical literature, however, are also ways by which the scientific and technical community communicates to an organization the highest intellectual standards

of the society. This is important not only to the research organization, but also, to the extent it succeeds, to the whole mission-oriented organization as a calibration point in setting professional standards. In addition, the observation of this sort of communication is a means by which management can evaluate the competence of work done in a research organization. Since much discipline-oriented research work has a technological relevance which only the future will reveal, this method of evaluation is quite important.

Another form of career development is the movement of personnel out of the research organization. This motion can be either back to the university or into other parts of the mission-oriented organization. There are other possibilities too, but these two are perhaps the most important in our time. The return to the university in certain cases is simply and unfortunately an expression of unhappiness with industrial research. But in other cases it is a healthy career development for the individual and it can have advantages for the industrial organization. In particular it forms recruiting contacts at universities who understand something about industrial research and it also puts people into university research work who have some awareness of the relevance of research to technology and even some inclination to work in fields with such relevance. As for the motion of personnel from phenomena-oriented research into the technologically-oriented parts of the organization, it is in general a good thing for careers, a way of injecting the most novel science into technological work, and a way for the organization to raise its standards in selecting people for promotion. Care in selection of personnel for such changes is required, but a sensitivity to the possibility of such changes is a way of gaining for the mission-oriented organization one of the real benefits of supporting research. It is well known, for example, that a significant number of members of senior management in mission-oriented technological organizations have started out in research.

The last broad aspect of career development I wish to discuss is periodic merit and salary reviews, which are of the greatest importance. The merit reviews provide an orderly and indeed inexorable mechanism for collecting all contributions and evaluating them at successively higher managerial levels. Patents are recognized here, as they are not always in professional society proceedings. Management reaches in this orderly way a consensus of views on which later personnel decisions can be based. Separate rate reviews provide a means of bringing salaries into line with merit review conclusions.

This review of opinions on various management topics is based on efforts to deal with such problems in a particular institution, Bell Telephone Laboratories. It is presented not as a definitive discussion, but as a means of indicating one point of view in management matters more generally, and for comparison with other viewpoints.

Questions and Answers following Dr. Galt's Talk

Question from Audience

Both Dr. Hollingsworth and Dr. Galt spoke very eloquently about getting the researchers. How about a word about the research managers?

Hollingsworth

My method is to look for volunteers. Someone has to come to me and say: "I want that job." I had just that decision to make recently and that's the basis upon which I made it. When it was all over, a man who was eminently qualified came to me and said: "I thought I was being considered for that job." I said "Why did you think that? You never so much as asked about the job." In contrast, the fellow who got the job came to me and said: "That is where I want to go, and I will make a success of it, if management will give me a chance. If I don't, in six months I will resign and you will never hear from me again." I'll go along with a volunteer like this.

Vollmer

But don't you often get volunteers, maybe, who may be good scientists, but not so good as managers? Is that ever a problem?

Galt

I am sure it is, and that's why I think that the senior management at the next level has to choose. If you decide that he's a volunteer who is just volunteering to commit suicide as a scientist, but has no concept of the problems of administration, you have to dissuade him gently. The point is that you need a man who does volunteer.

Van Atta

I would like to agree. I would say that the volunteer you don't want is usually a man who doesn't know what he is volunteering for. This is characteristic of my experience. Of all those you don't want, is a man who you know perfectly well won't do the job. I certainly find that there is some psychological demand on the manager; you ask him to do the chores and pass on praise to others. The question of where he gets his praise from is a little tougher in many ways. He has to get his satisfactions from a more sophisticated look at the research activity as a whole. I can put it in a more cynical way than that, but I think the point is that the volunteer who knows, who says the things that Dr. Hollingsworth just said, obviously is ready to go in there and do whatever has to be done.

Vollmer

Does it always require a top-level scientist to be respected as a manager by other scientists in any science lab, or can you have a man with some scientific background, but who is more oriented as a research manager?

Galt

I think the man must be highly competent as a scientist, so that he will have the respect of the scientists in the areas of which he is in charge, but you can compromise on his scientific ability in order to get a man with managerial talents. It is the combination of both sets of talents that counts. He must be respected technically, but as a manager, he uses his scientific background primarily to motivate other scientists.

Questions from Audience

Didn't the last two speakers mention the desirability of the research programs emanating from the senior scientists themselves? I am curious to know from a practical point of view how frequently this really happens in their organization.

Galt

I meant projects, not programs, which are groups of projects. Programs have to come from management, and projects are what make them up.

However, I feel a scientist needs as much freedom as possible in selecting specific projects to work on. We find that we are able, in a majority of cases, to endorse in terms of money the work that is promoted by senior scientists. You must understand that I am talking about a very small part of a mission-oriented organization, and it better stay a small part.

But, you try to set a framework for this project work -- a framework in terms of programs. The framework includes the budget. The man working in a program may have an idea that he doesn't think will cost anything but his time. In this case, a review is rarely necessary. However, if we calculate that it will cost a half a million dollars to do a piece of research, we may decide that we are going to have to reduce it. It is not an individual manager like myself who makes this decision -- it has got to go further. But if I can, I encourage the scientist to take some responsibility for originating the program, as well as carrying it out.

We also try to pick the scientist in such a way that his qualifications will allow him to fit into an important program area -- he's got this framework when we hire him. I think this comes back to the point that, when you are recruiting a man, the question: Is he interested in relevant activities? -- is something you have to think hard about, because people often say they are, even when they are not.

Pelz

This will probably anticipate some of the things I would have said later, but it seems appropriate at this point. I will be talking about a study that colleagues and I did on eleven research and development organizations -- five in government, five in industry, and one in a

university. One of the questions we were interested in was: "How much weight does the individual scientist or engineer have in the selection of his own projects, and how does the degree of weight he exerts on this process correlate with his effectiveness as a scientist or an engineer?" We asked him about the specific project that he worked on, not necessarily the program. How much of the weight in project decisions did he exert personally, and how much was exerted by his research management? Also we asked how much by non-technical management, how much by colleagues, and how much by clients. When we examined the amount of weight he felt that he himself exerted, in relation to various measures of his performance as judged by his colleagues and in terms of publications, patents, etc., we found some interesting answers. For non-Ph.D.'s in development-oriented organizations (these would be people with a bachelor's or master's degree in any of the sciences or engineering), we found that as individuals exerted successively greater weight up to about 80%, their performance continued to rise. When it got above 80%, there was some drop-off in performance.

For Ph.D.'s we found, rather surprisingly, a different pattern. I think I would have expected the same curve. Being a university man, my own feeling is that the more autonomy, the better. Actually, what we found was that for Ph.D.'s, both in research-oriented as well as development-oriented laboratories, the optimum amount of weight exerted by the individual was not more than 50%. Those Ph.D.'s who felt that they exerted 75% to 90% of the weight in selecting their own projects were only mediocre or below average performance.

In other words, the conclusion was that the scientists needed a certain amount of autonomy, but the Ph.D. often has too much. He may be going too far on trivial things -- working on problems which are perhaps not relevant either to science or to the mission of the organization. We found, for example, in our research laboratories, that the most productive scientists were not those who made the decision on selecting projects themselves, but who made it in collaboration with their colleagues. In development laboratories, the most productive Ph.D.'s again were not those who made the decisions by themselves, but made them in collaboration with either their supervisors or higher management. Collaborative decisionmaking seemed to be an effective balance wheel for Ph.D.'s.

Question from Audience

I have a question. I have a strong interest in mobility at the laboratory level -- that is in personnel shifting horizontally from one organization to another. What has been your experience with reference to the productivity of scientific and technical groups who have stayed at one place for a considerable length of time with little mobility of this type?

Vollmer

Are you speaking about individual mobility or for a group as a whole?

Answer from Audience

Individual mobility.

Galt

We certainly have had the experience of being successful in moving individuals from the research area to the mission-oriented parts of the organization. The president of Bell Laboratories originated in the research area himself.

Now I don't think I am answering your question, because I don't know the statistics, but when the man to be shifted is chosen carefully, the result can be positive both for him and for the organization as a whole.

Vollmer

Could I ask the gentleman's question perhaps in a different way? Both of you, Dr. Hollingsworth and Dr. Galt, have laid great stress upon the importance of selecting the right people, getting them from the universities, and so on. Do you select people whom you think are going to be staying in your organization? Or do you anticipate that they are going to move around a lot? There are some studies that have shown that if you go out after the new university graduate, you are going to get a man who is likely to turn over and make several job changes. Do you take that into account, and is that good or bad?

Hollingsworth

Well, you have to take what you can get. I don't care how much they move. We certainly go after such people, if they are good men. We know that they do move, and we try to get the best from them that we can, but we know that they will leave us in some cases.

Galt

We have some young Ph.D.'s on our staff for a one-year appointment. They may indicate that they want to stay longer, but we are only planning on their staying a couple of years. They bring us a new viewpoint. Many of them don't want to settle down; they aren't ready. So we let them go in good grace -- no hard feelings, and maybe they will come back some day.

Occasionally at the end of the first year, it may be obvious that a man is ready to become a permanent employee, and that we want him to stay.

Hollingsworth

Yes, that kind of mobility I will promote. I think in terms of five to ten years. I don't know how to predict whether a man will

move or not in this period. I think it depends on how successful he is, and therefore how many offers he gets.

Question from Audience

My question is, in a monopolistic organization, such as Bell Telephone, how do you maintain this type of climate you have been describing, as opposed to a more competitive type organization?

Galt

I guess the first thing I would say is that perhaps it is for others to be the judges of the vigor of our research and development program. I would claim that it is high.

The other thing is to say that we are dealing here with part of the organization which is not involved directly in competitive commercial questions. The fact is that the Bell System does have competitive problems. But it also has a monopolistic position in certain areas. The Bell System does not commit itself to buy equipment from Western Electric. It is quite free, and does in fact, buy a great deal of equipment from other suppliers. In short, if the Bell Laboratories is unable to develop equipment which Western Electric can then produce at competitive prices, they will turn elsewhere.

However, I think I ought to say that the phenomena-oriented research organization is quite removed from these questions. Its vigor is much more determined by its relationship to the scientific community. This is something which, I think, we have achieved by the selection of personnel who are trained to work with the scientific and technical communities, in competition with scientists at universities.

Personnel movement is relevant to this. We are a happy hunting ground for recruiting for university physics, chemistry, electrical engineering, or mathematics departments.

PLANNING PHENOMENA-ORIENTED
RESEARCH IN NASA

by

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Basic Research Versus Applied Research

The term "phenomena-oriented research" is well chosen to describe an important basic aspect of applied research. However, there is sometimes a tendency to make a distinction between basic research and applied research, and to narrow the category of applied research to the point where it is hardly distinguishable from advanced development. Logically, it is pure research that should be contrasted with applied research. This distinction does not depend on the research itself but on its motivation or its relation to the interests of the organization. The same research project might properly be classified pure in one organization and applied in another.

Applied research conducted by a good research man in a favorable environment is likely to have a basic content. The rewards from good applied research can then be of two kinds: providing a stronger scientific base for the development of needed technology, and advancing basic scientific knowledge. Pure research must be justified solely for its contribution to scientific knowledge.

When Congress or the Bureau of the Budget ask NSF, NAS, OST, or PSAC how funds should be distributed for pure research among the various fields of science there is only one safe answer: In proportion to the number of competent research scientists in these fields and the cost of their equipment. This answer would lead to greatest effectiveness since it would make best use of the research talent available.

There are some indirect controls, however, on the distribution of research scientists. Actually, we have been exercising these controls rather effectively -- however wisely -- for some time. Job opportunities for applied research and development in a given field influence the degree of student interest in that field and the size of the corresponding academic faculties. Also greater applied effort in a field tends to call attention to its unsolved fundamental problems. It is therefore through emphasis on applied research and development that pure research effort can be influenced indirectly to the degree that is meaningful in terms of national goals.

Agencies with a specific responsibility for the support of pure research are few, and funds available for such support are very limited. Perhaps partly for this reason pure research has enjoyed greater prestige than applied research, even though the first pure research was done by men with development problems, who were diverted to the purely scientific aspects of the subject by a healthy intellectual curiosity.

An advantage of this prestige rating, and perhaps also a contributing factor is that many of our best scientific brains have been attracted to the very difficult basic problems. Another effect of this prestige is the tendency to label applied research as basic research. As a result, many mission-oriented organizations make a distinction in their technical programs between applied research and basic research activities, presumably implying that the basic research does not have a demonstrable or likely application to their missions.

If applied research is defined more broadly to include research that may have long range and rather general application to the interests of the organization, and if applied research is expected to have a strong basic scientific content, then it can be argued that a mission-oriented organization is justified in supporting only applied research with funds allocated by Congress for specific missions. However -- let me emphasize -- this probably is not an argument for changing existing research programs, but rather for putting the descriptive terms on a more rational basis and thereby sharpening up the justification for research funds.

To the extent that pure research can be planned, it must be done by intuitive and experienced research men on the basis of experimental or theoretical leads. There may be scientific questions to be answered, alternative theories between which a choice must be made, or discrepancies to be cleared up. These problems grow out of science itself or the recent work of scientists.

In contrast, the motivation for applied research grows out of the possible application of its results to mission-oriented problems, whether specific and short range or general and long range. Hopefully, such applied research will lead sooner or later to advanced development, and an expansion of the technological base for future missions. The planning of applied research, therefore, must be based on its possible applicability to fields of technology important to the organization.

In what follows, some techniques will be described for selecting areas in which research competence should be built up within an organization, and for defining rather specific problems within these areas, hopefully without inhibiting the method of attack or unduly restricting the nature of the solutions. This long range

planning must involve the entire organization from top management to the individual research man, because each organizational level has its part to play. If top management is not involved the research plan will lack meaningful broad objectives and adequate support; if the individual research man is not involved the conception of research projects will lack imagination and substance.

R&D Versus Engineering

It has been my experience that a research effort in a mission-oriented organization will be more fruitful when it is combined with advanced development than when it is given "ivory tower" status. One reason is that, in the exchange between the research man and the development man, the research man learns of basic problems and the development man learns of opportunities for application. Another reason is that the more tangible output from advanced development is much more understandable by and salable to the engineer and the mission planner.

There are major differences between R&D planning and engineering planning, especially the planning of complex systems or missions. In both fields there must be long range and short range plans. In both fields planning should contain elements of practicality and elements of innovation.

But the engineering planning must emphasize a practical, realistic approach with just a touch of imaginative boldness. In contrast, R&D planning must emphasize a bold and imaginative approach aimed at major technological advances with just a touch of practicality. In keeping with the theme of this session as well as my own interests, I will devote my attention entirely to R&D planning.

Prerequisites to Research Planning

As mentioned earlier, effective research planning must involve the entire organization from top management to the individual research man. In other words, the detailed plans can not exist without the organization; the plans can develop only as the organization is established. Phenomena-oriented research requires an organization based on scientific or engineering disciplines rather than on missions or systems. If the research is to have a basic scientific flavor, the staff comprising the research laboratory part of the organization must have a strong academic background. Other prerequisites to research planning are the following:

- a critical mass of capability in each significant subject matter area;

- an adequate range of disciplines represented in the organization to cover its general area of interest;

- a stable research budget with a workable means of allocation among disciplines and projects;

patience on the part of top management during the start-up period;

freedom from detailed short-range engineering and mission responsibilities;

close communication up and down the line designed to relate plans to goals.

If most of these conditions are reasonably well met, the research laboratory will become almost inevitably a great pillar of strength in the long-term support of its organization.

Levels of Participation in Planning

In an industrial corporation, top management must decide whether there is to be a corporate research laboratory, how the activity is to be supported, and at what level. Assuming that corporate objectives have been defined, top management must relate the goals of the proposed laboratory to these objectives. After the laboratory is established, two-way communications with its director must be maintained, long term performance evaluated and support assured.

So in NASA, Mr. James E. Webb and his associates made these decisions regarding the Electronics Research Laboratory. The decision to create ERC was based on a firm conviction of need and has not been affected by current constraints. Headquarters prime responsibility for administration was assigned to the Office of Advanced Research and Technology. Further planning and study at Headquarters established the ultimate size of ERC, its location, and its major areas of activity.

At the next management level, the research director must further define technical areas of interest and the relative emphasis among these areas, must establish the major elements of the research organization's administrative and technical structure, and must attract competent personnel into key positions in his office or reporting directly to him. The research director must be the link between scientific and support activities, and the spokesman for the research organization with top management.

Again at ERC, the assistant directors assume responsibilities for specialized technical or administrative areas. In their individual capacities and as a part of the director's office they participate in Center-level management decisions and in more detailed research planning, organization and staffing. The technical assistant directors, working with the heads of the laboratory areas for which they are responsible, with program specialists at NASA Headquarters and with a knowledge of the long range needs expressed by other NASA Centers, select long range objectives for high priority attention. These objectives must be so chosen in number and achievement level to be within

the resources available. They must be based in such a time frame as not to be competitive with work already underway at other Centers, but capable of contributing to long range NASA problems already visualized. On this basis most of the long range technical objectives have a five to ten year period for achievement.

These generalities can be given more substance by reference to my own area of responsibility, electromagnetic research. In terms of ERC structure, this means the programs of the Microwave Laboratory and the Optics Laboratory. In the Microwave Laboratory, long range objectives emphasize deep space communications, communications through re-entry plasma, satellite-aided earth survey, satellite-aided communication and navigation, direct broadcast from satellites, and communications and collision avoidance for the advanced supersonic transport. These mission-related objectives convert into several research and advanced development activities:

- Propagation research at microwave and millimeter wave frequencies in the earth's atmosphere, in the atmospheres of other planets, in interplanetary space, and in plasmas;

- millimeter wave technology development of radio frequency power generation devices, phase shifters, and circuits based on electron tube and solid state techniques;

- advanced development of array antennas for spacecraft and ground use, with emphasis on distributed solid state amplifiers in the spacecraft, distributed receivers on the ground array, and an intimate relationship in both cases between array antennas and digital computers.

Similarly, in the Optics Laboratory, long range objectives broadly relate to high data rate space communications and high resolution imaging and mapping from spacecraft. Again these future mission capabilities indicate the need for:

- propagation research at optical and infrared frequencies;

- development and space qualification of lasers and laser-related communication system components;

- research on laser and optical materials and on nonlinear radiation effects;

- advanced development of holographic concepts and techniques;

- advanced development of large diffraction-limited optical elements.

In each of these areas of research and development five or ten year goals are established and change only in minor respects from year to year. In each area, however, new tasks are established each year for prosecution in-house or on contract. These changing tasks are a measure of progress toward the long range goals. The funding of these task provides the finest unit of budgetary control over the technical program; this control is exercised at the level of the Center director.

The Conceptual System as an Aid to Planning

In many cases the physics of a situation is so incompletely understood, or the optimum systems application of a field of technology is so uncertain, that a clear relationship of a specific technology to a specific mission function can not be established. In these cases phenomena-oriented research must be pursued on a broad base, and technology development must not be allowed to become unduly specialized. As scientific understanding of the situation improves and as technological capabilities are defined, it becomes possible -- or in some cases necessary -- to adopt a particular technological approach to meet a future mission function.

This definite correlation of a future technological potential to a future mission need can be called a conceptual system. The conceptual system visualizes elements of future technology that could accomplish the various systems functions within a specified time frame with a reasonable application of research and development effort. The conceptual system, in turn, defines more clearly the direction of the R&D effort if the systems needs are to be met. Thus a portion of this effort can be more clearly defined in terms of problems to be solved or component capabilities to be achieved. Technological development can be put on a collision course with future system needs.

In terms of long range planning the conceptual system permits a portion of the research and technology effort to be more strongly focused, it permits more convincing budgetary requirements to be stated, and finally it provides a better measure of R&D effectiveness. If, as the selected time approaches, research has indeed eliminated the unknowns and development has provided the technology, the conceptual system has justified itself as a reliable guide for R&D. At this point an experimental system can be designed on the basis of the new techniques and components, and subjected to feasibility tests.

The successful feasibility demonstration of an experimental system permits the responsible mission designer to give serious consideration to the techniques and components utilized by the system. The associated research can find early application,

advanced technology can find its way into engineering. Only then can the giant step be taken to the prototype system. But the major effort of the prototype system can be undertaken with much greater confidence as a result of the experimental system, and is likely to incorporate much greater sophistication from a well-directed R&D program as a result of the conceptual system.

Questions and Answers following Dr. Van Atta's Talk

Vollmer

In your remarks on technical matters, Dr. Van Atta, who makes the decisions on these kinds of matters in your laboratories? Have you been talking about the decisions that you make as the director, or the scientists make, or you make together? How do you decide whether or not to undertake a new project?

Van Atta

These are mostly decisions which I initiate, subject to critical review by people down the line, and subject to change. I find that I am wrong about details often enough so that I have to be, again as Guil Hollingsworth said, very willing to make changes and to be subject to the influence of facts.

Vollmer

This morning a point was made by a member of the audience that research and development are somewhat "incompatible phenomena". I just wonder what the speakers would have to say on that point here -- particularly Dr. Van Atta, who told us a great deal about the inter-relationship of research and development. Do you agree, Dr. Van Atta, that these are two different kinds of things that require different kinds of people, or would you disagree?

Van Atta

There are those people who would prefer to have the imagination and the interest that allowed them to work on a very long-range problem with broad interest. Then there are those people who feel a little more comfortable with shorter range problems. In a research laboratory once in a while where there has been some work on lasers, you will find a person, for instance, who feels that he would like to go on with this laser work and develop it into a product line. Then someone from the research laboratories goes around to one of the operating divisions and says: "Allow this fellow to get started and build up his staff for the next six months". If you will promise me in writing that you will take the product over at the end of six months, then you can start something like that. The trick is to recognize that a mission-oriented organization is not running a research laboratory purely as window dressing. It is running it with the idea that there is going to be some solid return. In general, I think you will find that a well-run research organization is going to give more in return than a corresponding engineering effort, and more in terms of the return on investment, provided that the research is well planned, and especially that it is well integrated into the larger organization.

Galt

I would like to develop a little further some of the things that Dr. Van Atta has said. It seems to me that one can describe the process he mentioned by saying that the people who do development are sometimes the same people who did research earlier in their careers. But they are different now, all right. In fact, the same person may change his mind in the course of a career and move into development organizations. That is part of the game, and is good, by and large, I think. That is to say, in the field of science and engineering, I think one can often do a good thing by starting a man in a research activity and letting him move -- in fact urging him to move -- towards development in the engineering areas. It doesn't always work, but Dr. Van Atta just described a way in which it sometimes works. From my point of view, it is a good thing.

Question from Audience:

Do you ever have movement the other way, that is engineers perhaps going into research, or people who work in development who now would appreciate the opportunity to go back into research? Could there be movement both ways?

Galt

From my own experience, I think the amount of movement the other way is not so large. That is not always because people do not want to move the other way, but it is because management usually doubts that it is a good idea. I think that this direction for the change is a natural extension of a man's adjustment, starting at the university and moving toward the industrial or governmental research organization and its problem areas.

Vollmer

Let me expand a little further on this point that Dr. Galt has just made. The big information transfer is usually from research to development or engineering, and this information is always hard to transfer. In fact, the nearer you get to research, the harder it is to transfer. The easiest way to move the information may be to move the person.

PLANNING PHENOMENA-ORIENTED
RESEARCH IN AFOSR

by

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The AFOSR Mission

Before I discuss the planning of the research program at the Air Force Office of Scientific Research, it is important that I first explain AFOSR's role in the Air Force research, development, and systems acquisition program.

AFOSR is part of the Office of Aerospace Research, a Separate Operating Command of the Air Force, with overall responsibility for the Air Force's corporate research activity. OAR has a budget of approximately \$90 million annually for research. AFOSR's share is approximately \$40 million.

The AFOSR research program is accomplished entirely through contracts and grants. Currently we have 930 active work efforts and these are being accomplished in universities, non-profit and industry research organizations. Other activities of OAR are the AF Cambridge Research Laboratories and the Aerospace Research Laboratories, both in-house laboratories with an associated contract program.

The systems development responsibilities in the Air Force rest in the Air Force Systems Command. AFSC has an annual budget of around \$8 billion, over \$3 billion of which is for research, development, test and engineering. This organization conducts a great variety of applied research, exploratory development, advanced technology, and systems engineering programs. It also has responsibility for initial procurement of new systems for the inventory.

Perhaps I can best describe the AFOSR mission by referring to Figure 1. We find it important to recognize that science and technology activities fall into two rather diverse groups. One we have designated as phenomenon-oriented research having as its primary goal the increased understanding of scientific phenomena. The other activity is technology which has the goal of producing products, devices, materials and systems.

We find that communication within the scientific community takes place very well. Scientific goals are pursued, frontiers of science developed, and very rapid, effective communication

takes place among various members of each "invisible college", the group of scientists spread around the world pursuing a given specialty. Likewise, we find that technology advances typically have their obvious origin within technology and here again good communication takes place (1).

This model is only a first approximation because when one looks deeper (2, 3, 4, 5), one finds many possible important avenues of interplay between science and technology. Some of these are actively utilized; others need to be further developed. The AFOSR program is designed to help assure that these bridges between science and technology are as effective as possible.

The role of AFOSR is to help assure that the maximum possible benefits accrue to the USAF from the research activities that may be accomplished in the scientific community. Toward this end we engage in two types of activity.

The first main function is to provide communication between the scientific community and the Air Force. This is a two-way communication -- needs to the research program and scientific information to the user. The AFOSR project scientists play the key role in this communication or coupling activity. In addition, part of what we purchase through contracts and grants is primarily designed to provide communication. This part refers not only to the symposia we sponsor, but to the connecting-type research which allows us to keep abreast of a variety of scientific areas largely supported by other agencies, but nevertheless important to the Air Force because of rapidly emerging scientific developments.

The second function is to support scientific research chosen because of particular interests of the Air Force. We pursue this support in a manner calculated to colonize scientific activities of special importance to the Air Force. The selection of these areas may be motivated either by seeking to pioneer new fields of science holding out high promise for generating the new knowledge from which new technologies or new operational possibilities may evolve or it may be motivated by helping various development or other user groups solve certain difficult classes of important problems by providing a fuller understanding of phenomena behind them.

The role of AFOSR is quite similar to that of phenomena-oriented research activities in other mission-oriented organizations, including industry. The role of this general class of research activities has been discussed in detail elsewhere (6, 7, 8).

Planning and Scientific Choice

One of the questions which often arises in the discussions of policies for Federal support of scientific research is that of determining the distribution between the various fields of science (8, 9, 10). The growing discrepancy between the funds appropriated for the support of research and those required to support the available quality research proposals increases the importance of this question. An overall objective is to assure that the decisions as to the distribution of funds are made in a manner which maximizes the contribution to the solution of society's problems and at the same time assures that the quest for new scientific knowledge proceeds in a completely viable way.

It is important to note the key role which the research activities supporting mission-oriented agencies and other similar activities play in the answer to the question outlined above. The application of the proper planning procedures by AFOSR and other activities results in a scientific research program tailored to the needs of each agency. At the same time, when considered as a group, these several research agencies provide support for a great variety of scientific research on the forefront of knowledge.

There are, of course, special planning considerations appropriate for AFOSR and these other research agencies that support a mission-oriented organization. On the one hand, a proper planning procedure brings about choices of broad scientific fields and of specific work efforts within these fields such that the scientific research program has a "center of gravity of interest" which meets the needs of the agency it supports (8). At the same time, the distribution must not be restricted by too narrow a definition of relevance. It must be recognized that some areas of science have special importance for several (perhaps all) of the mission-oriented organizations and that support of these areas by more than one research agency can be important both to provide these organizations communication with the fields and to assure that there is adequate support in these vital areas. Further, the distribution must recognize both the uncertainties in our knowledge of the scientific results which will be obtained and even more so the unexpected avenues of utilization of new scientific findings and therefore the unexpected relevance to the mission of the agency.

AFOSR Planning Methods

The challenge in planning is to optimize the distribution of our resources -- our manpower and money -- among our various activities in order to make the best contribution toward our mission objectives.

One problem is to properly distribute the time spent by our fifty-five scientific personnel. Certain time must be spent in following up the opportunities continually emerging in new science, in carrying on a dialogue with Air Force using agencies, in managing the contract and grant program, and in devising special activities which provide the communication between science and technology which I mentioned. The distribution of effort among these several functions is a very important matter.

The other principal planning problem is the establishment of the distribution of funds among the various possible fields of scientific research.

Inputs appropriate for consideration in planning the AFOSR research program come to us in many ways. Let me first mention information about the long-range needs of the Air Force which comes to us from top management. The Secretary of Defense and of the Air Force, the Chief of Staff of the AF, and others, make speeches and also provide various internal documents. "The Plan", USAF Planning Concepts, a fifteen-year projection prepared by the Deputy Chief of Staff, Plans and Operations, is one such document that is important to us. From "The Plan" we learn about the projected military tasks -- tactical and strategic warfare, space operations, etc. We carry on a dialogue with the persons responsible for preparing "The Plan", thus furthering our understanding of its implications for our research planning. At the same time we also influence "The Plan" by helping elucidate the implications of emerging scientific opportunities on future AF operational plans.

We also receive important guidance from AFSC. Two principal inputs are the AFSC Planning Activity Report and the Technical Objective Document. The latter, for example, sets forth the AF interest in each of the thirty-eight technical areas. Figure 2 lists typical areas.

Using these various sources of information, we have developed a list of technology areas to be considered in assessing the relevance of research. The relevance is studied with the aid of a large matrix showing the relationship of the technology list to a similarly detailed list of the scientific areas. It is very important to note that the technology list includes both the areas designated by AFSC technology organizations and those which we add as a result of other inputs. For example, our list includes activities in support of military assistance programs, personnel management and training, logistics, and other needs which we find arising from other parts of the Air Force.

These additions are very important, we feel, in carrying out the corporate research function. Our studies of science and technology interactions convince us that it is very misleading to expect a neat flow of research needs from technology just as it is unrealistic to expect a simple flow from a scientific discovery to applied research to development, etc.

Thus one source of "grist for the mill" of the OAR and AFOSR planning activity comes to us from higher headquarters and from plans of various operating commands. The obtaining of these inputs is the responsibility of line management and their supporting staffs.

In parallel to these activities there is the continual concern for planning at the individual program manager level.

An AFOSR program manager typically has about twenty active contracts and a million dollar annual budget. Each program manager has one or two subareas within his responsibility and Figure 3 shows a few typical areas. Each staff scientist engages in a variety of activities. Figure 4 lists these activities. His task, of course, is to optimize the use of his own time and the distribution of his research support. So as in all activities we come down to the single most important item being the selection and motivation of the staff. It is clear that his personal knowledge of both the emerging opportunities of science and the DOD needs are highly important aspects of his qualifications. It is also very important that he develop appropriate meaningful personal contacts with those persons throughout the AF most interested or most likely to be interested in the research program for which he is responsible.

There are several techniques which the individual program managers or group of program managers have found particularly useful in planning. Let me quickly outline a few of these.

We just sponsored a workshop on Fundamental Problems of Future Aerospace Structures in which we brought together fifteen designers from diversified aerospace industries to present their views of research needs in structures. AFOSR staff, along with the Franklin Institute, will analyze these presentations to help us plan our on-going research program.

We are currently holding a biweekly seminar on research problems to support limited conflict. The primary purpose of this seminar, which consists of a wide variety of speakers having intimate knowledge of limited conflict problems, is to increase the sophistication of AFOSR staff members in selecting appropriate long-range research problems to support that AF mission.

In our combustion dynamics program we run annually a meeting of all of our contractors along with representatives from the AF R&D organizations. At this symposium contractors present their research results and the AF representatives present research problems which they see.

The AFOSR staff members participate in ad hoc studies of research and technology utilization for operational needs. Two current examples are the feasibility studies of the scramjet propulsion system and the new emphasis on special air warfare. The knowledge which we obtained through this direct participation in operational problemsolving feeds directly into research planning accomplished by individual project scientists.

Another fruitful technique is the use of in-house advisory committees. Carefully selected members from throughout the Air Force technology community meet on a semiannual or annual basis with groups of individual program managers.

Other significant examples of special techniques which I will simply mention are state-of-the-art reviews and interagency coordination groups containing both research and development managers such as the Interagency Chemical Rocket Propulsion Group.

The third main, and absolutely essential, source of information for planning of our program is the information on the emerging opportunities for scientific research. An important source of this type of information is the series of reports on the opportunities and needs of basic science, discipline by discipline, under the auspices of the National Academy's Committee on Science and Public Policy. Our nine research evaluation groups, one for each of our principle scientific areas; and the scientific advisory group for OAR, which overall include approximately 100 of the Nation's leading scientists, provide another very fruitful source of guidance.

The most vital source of this information is the current knowledge of each AFOSR project scientist of the research area for which he is responsible. He is in an ideal position to be knowledgeable of the emerging fields of science because he continually receives unsolicited proposals from scientists seeking support. The originators of the 2,000 formal proposals and several thousand informal proposals which AFOSR receives each year are ready, willing and able to provide this education to the AFOSR staff. This activity supplements in a very effective way the other professional activities, including sabbaticals, by which the AFOSR project scientists seek to be knowledgeable of the emerging fields of science.

Working with the six managers of the principal scientific directorates, my immediate staff office in charge of planning, and other key AFOSR program managers, I carry on an essentially continuous dialogue relative to the balance of our activities, both among scientific areas and in the use of our time, for example, coupling vs. research contract management.

Once a year Headquarters OAR calls us and our counterparts from the other parts of OAR together to formulate the OAR Five-Year Plan. Also present are selected members from Air Force organizations using research. This plan is a comprehensive document which sets forth the specific scientific areas in which it is felt research should be supported by OAR. Let me remind you that the information which I have described above, being of various types and from various sources is the input to the Plan. The quality of the Plan in the long run, of course, must be a direct function, both of the quality of the input and of the intelligence and skill of the OAR personnel who formulate it.

This OAR Five-Year Plan, in its published form, becomes the guidance which AFOSR receives from its parent organization. It is revised annually and in this manner it is kept viable in terms of responding to new scientific opportunities or of improved understanding of future AF needs for research.

Each year as resources are made available, decisions as to the distribution of funds between various scientific areas are made, giving due consideration to the Plan. Over the years gradual changes occur in the distribution among the several fields of science we support. In turn, as the individual program manager considers proposals for new work and renewals, he makes further changes. Incidentally, the changes which occur within the individual programs are often quite significant, much more so than the changes in funding levels between broad scientific areas. It is important, however, to note that these changes are made in a manner which recognizes the long range nature of the individual research efforts, providing adequate time for phase out of our support and assistance in changing sponsors whenever possible.

Conclusion

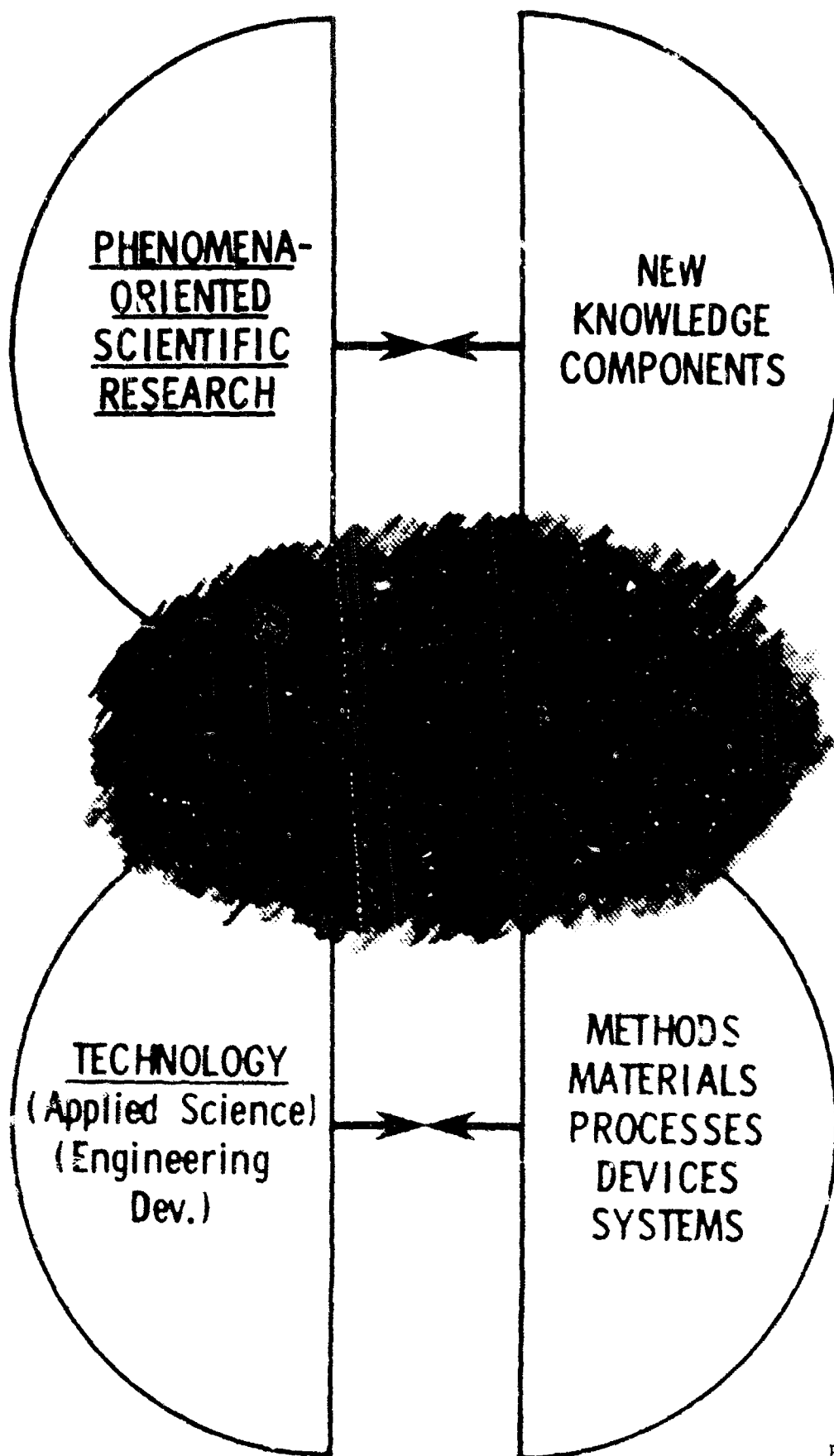
We have no magic formula for doing our planning and we suspect that none will ever be found. We do believe, though, that searching for answers to these all important questions in responsible and intelligent ways further optimizes the contribution of our activities. For example, during the last four years we have put a lot more emphasis on the communication function than we did previously. Similarly, we have built up the Behavioral Sciences area, we have cut back on major areas of Mathematics,

Nuclear Physics and others, for example. We have dropped many individual work efforts and picked up thousands of new proposals. These and many other actions have kept our program dynamic and effective.

In summary, we are keenly aware at AFOSR of the necessity for the rather complex class of activities characterized above, which we group under the general heading of planning. Certainly to maximize the impact of our support of the Air Force through phenomena-oriented research, we are faced with many choices. While scientific excellence is always a prime consideration in what we support, we know that in general society's problems (in our case DOD problems) do not come neatly packaged in terms of scientific disciplines. We are bringing the relevance and other considerations into play by many means and we continue to search out additional means to further improve our effectiveness in this area. One of our most active areas of current interest is to try to determine the value of technological forecasting as an input to research planning.

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EXAMPLES OF TECHNICAL AREAS OF INTEREST TO AFOSR

AEROSPACE MEDICINE

AVIONICS COMMUNICATION

FLIGHT CONTROL

MATERIALS

ROCKET PROPULSION

STRUCTURES

SPACE ENVIRONMENTS

EXAMPLES OF SUBAREAS IN AFOSR PROGRAM

	<u>No Efforts</u>	<u>Annual Rate</u>
PLASMA PHYSICS	11	\$ 400 K
SURFACE PHYSICS OF SOLIDS	12	400 K
ORGANOMETALLICS	12	280 K
CONTROL THEORY	21	540 K
TEAM UNIT PERFORMANCE	11	225 K
STRUCTURAL MECHANICS	25	700 K
COMBUSTION DYNAMICS	33	1200 K
INFORMATION IDENTIFICATION AND CLASSIFICATION	15	311 K
COMMUNICATION AND INTERACTION WITH FOREIGN POPULATIONS	14	300 K

ACTIVITIES OF AFOSR STAFF SCIENTISTS

MATCH NEEDS OF AF AND OPPORTUNITIES OF SCIENCE.
COMMUNICATE NEEDS TO SCIENTIFIC COMMUNITY.
RELATE AF PROGRAMS TO NATIONAL RESEARCH SUPPORT PROGRAMS.
VISIT AF ACTIVITIES (INDIVIDUAL VISITS, RTD OPEN HOUSES, ETC.)
EXPLOIT RESEARCH RESULTS.
PROVIDE CONSULTANTS TO AF ORGANIZATIONS.
VISIT CONTRACTORS AND GRANTEEES.
ACCOMPLISH ROUTINE CONTRACT AND GRANT ADMINISTRATION.
EVALUATION OF UNSOLICITED PROPOSALS.
AD HOC FUNCTIONS (IR&D EVALUATIONS, PLANNING PANELS, ETC.).
OTHER PROFESSIONAL ACTIVITIES (SPEECHES, CONSULTING, ETC.).

Questions and Answers following Dr. Price's Talk

Question from Audience

I would like to ask Dr. Price what he considers the primary role of in-house laboratories; and, in connection with this, the relative support that is given to the in-house activities versus out-of-house activities; and thirdly, the criteria by which jobs are selected for the in-house laboratory versus the out-of-house branch.

Price

You have asked a complicated question, one that's difficult to do justice to here with a short answer. Further, you are asking the wrong person since the program for which I am responsible is all accomplished through contracts. I have no option to utilize in-house laboratories. I do want to say though that the in-house laboratories are very important to the Air Force in several ways. They do high quality research making important contributions to the Air Force and the pool of scientific knowledge. At the same time they are helping solve a scientific manpower problem. Federal agencies have a very substantial need for persons to manage R&D. It is very important that ways be found to continually upgrade the quality of these managers. The main way we have available to accomplish this is to "grow our own." The in-house laboratories play an important role here. At the same time they are doing very important specific jobs. For example, jobs requiring a degree of coupling to other Air Force programs which is not practical if the research is contracted. Also the in-house research laboratories provide an important window to science being accomplished in the scientific community out of the DoD.

Vollmer

Let me just amplify the gentleman's question. I don't want to embarrass you on this, but it seems that one of the fundamental questions in the planning process for any area of management is whether you make something or whether you buy it from the outside -- that is, whether you do it in-house or do it outside. And so it seems to me that this is a very significant question, and I don't know whether anyone up here on the platform has any guidance on this sort of thing. Here we are talking about phenomena-oriented research. How much of it do you try to buy from people at universities and places like this where we know that they do a lot of this kind of research? Maybe you can sponsor some of it on the outside, but maybe there is some of it that you ought to do on the inside for reasons that Dr. Price pointed out here. But how do you determine the relative balance of this kind of thing? Does anyone have any comment to make?

Van Atta

I would like to add to that, Howard, that we in NASA have an in-house program and we have contracts and grants out-of-house. We do not let contracts or grants in areas where we do not have any internal competence, for reasons that should seem clear enough. And we attempt to avoid having any person responsible for the monitoring of more than one contract, the idea being that he is there to do his own work and should not have too much of his time diverted to contract monitoring because he is not primarily a contracts monitor.

In the early stages of our work, where we are a fair amount removed from hardware, we depend heavily on universities with our grants or contracts. We find that working with the university you don't have that pressure you get particularly with hardware, that we encounter sometimes with the companies. Now later on as we find ourselves moving into hardware areas, then this is the job we would like in many cases to give to industry. Or where we must have a number of certain elements built, this is a job we like to give to industry. Or if there is in industry an outstanding team with which we would like to be associated, this would be a reason for going into industry in a special case.

Price

I would like to say that this also fits the Air Force experience. In addition I personally think that laboratories like the Naval Research Laboratories and the Aerospace Research Laboratories of the Air Force are extremely important to the services in conducting phenomena-oriented research, for reasons very similar to those at Boeing and BTL.

Question from Audience

But in all this, most of the work that is done is on hardware, and is not sponsored by NRL. Basically they are a consultant organization to the Navy, so it would seem then that there is quite a problem of communication from research to development in this area. In your own case, is there one?

Price

Well, the problems which NRL and ARL have in communications are exactly analogous to the problem Guil Hollingsworth explained for the Corporate Research Laboratories of Boeing, and John Galt was talking about for Bell Telephone Laboratories. In AFOSR the responsibility for this communication lies with our research program managers, who turn out to be analogous to the persons in second-line management in John Galt's organization. They are the persons who perform the interchange between the needs of the Air Force and its scientific programs. Yes, they have problems, too, but recognize that for the problem we have in communication, we have a very big "plus" of being able to

persuade university scientists who have their choice of support -- real top people -- to work for the Air Force, when we support their areas of physics, chemistry, etc. They won't work for a developmental organization per se, so we trade off the communication problem for the fact that we bring extremely fine talent to our programs. And likewise the NRL and ARL laboratories attract top people to the in-house staff. We feel that the communications problem is worth living with. As a matter of fact, we can't avoid it, if we are going to be able to attract our share of the most creative scientists. These persons are typically found among those who initially say "Look, science is what I want to do best, and I am willing to do it for the Air Force, if that is what you really want done." Incidentally, please note that organizations with phenomena-oriented research activities are having good success in motivating those persons to be concerned about the mission of the organizations and to contribute to it in ways which both the scientists and persons having the problem agree to be very constructive. Several of the speakers today have touched on this. I think that probably Dr. Pelz will be elucidating this point further.

SOME FINDINGS FROM STUDIES OF SCIENTISTS IN ORGANIZATIONS

by

Donald C. Pelz

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University of Michigan

As a social scientist -- specifically a social psychologist -- I have been working for a number of years in the field called organizational behavior. Several years ago I began looking at a special kind of organization, the research and development organization, and began to ask whether one could find factors in the climate of R&D laboratories which might affect the technical performance of their members. My first study was at the National Institutes of Health. Subsequently my colleagues and I made a larger study of eleven R&D organizations including five government laboratories doing both applied and basic research in basic physical sciences, weapons, and agriculture; five industrial organizations in electronics, pharmaceuticals, glass and ceramics, and electrical machinery; and one large midwestern university in which seven departments participated.

Methods

In each of these laboratories, comprising some 1300 scientists and engineers from the bench scientists up through the research director, we sought several measures of the scientific and technical performance of individuals. Mainly we relied on the judgment of their colleagues within the organization. Within each laboratory I would ask management to let me have an hour of time of a number of the senior staff, both non-supervisory as well as supervisory. I asked them to imagine that they were serving on a committee of their professional society to give an award for outstanding contribution to that field of knowledge within the past five years. This they did by sorting cards with the names of staff members into piles, indicating who in their opinion had made the most outstanding contribution, and who had made lesser contributions. This constituted our major measure -- technical or scientific contribution as judged by colleagues.

There were other measures. The judges were asked to indicate which individuals in the laboratory had been the most useful over the past five years in contributing to the organization's objectives -- whether through research, administration, technical services, etc. In addition we also obtained the number of papers published, the number of patents and patent applications, and the number of unpublished reports within the past five years.

To study the effects of climate on performance, we did not simply ask the members of these organizations what kind of climate they preferred. Rather we asked them what kind of climate they actually experienced. For example, we asked them how much influence they exerted in the choice of their technical problems -- not how much influence they would like to have, or how much they thought was effective, but how much weight they actually had or thought they had. Then we looked at the various measures of performance of these individuals, to see how performance varied as the amount of influence varied.

The results were recently published in a book, Scientists and Organization, by myself and Frank M. Andrews (Wiley, December 1966).

What were some of the things we found? At a sociological meeting last August I tried to summarize what is in the book; a revised version of this summary is going to be published in Science. It seemed to me that many of the findings could be described by the general statement that scientists and engineers in our study were often effective under conditions that were not completely comfortable -- conditions with pressures in apparently opposite or antithetical directions. These individuals must have experienced some degree of tension. Since they were productive and creative, I would like to call these "creative tensions," and will try to illustrate a few.

Diverse R&D functions

One set of results came from a question on the extent to which an individual allocated his time to different kinds of research and development activities. One category of activity was research directed toward the discovery of general knowledge relevant to a broad class of problems. This, I think, is phenomena-oriented research. Some might call it basic research. A second category was research directed toward discovery of specific knowledge for the solution of particular problems. This could be called applied research.

There were two categories of development -- the improvement of the existing product or processes, and the invention of new products or processes. There can be some question as to the dividing line between these, but I think one can distinguish between long-range or exploratory development aimed at inventing new devices, as against shorter-range development aimed at improving existing devices.

There was a fifth category to some extent a catch-all: technical services to help other people and groups. You have in your laboratories many people whose usefulness consists of being storehouses of expertise. Also included here are individuals performing standardized services such as testing or analysis.

An individual might spend some of his time on all five of these R&D functions, four of them, three, two, or only one of them. The question to be studied is: to what extent should a technical man concentrate on one or two, or spread his time over four or five if he is to be effective either in usefulness to his organization or in contribution to science?

I might say at this point that before starting our analysis we tried to break down the rather heterogeneous set of laboratories into more homogeneous sub-groupings. There has been some discussion at this session as to whether research and development fit into the same basket or not. Are they similar types of activities or different? Should the management of them be similar or different? We wanted at the outset not to simply lump all scientists and engineers together, but to put them into appropriate distinct groups, and analyze these groupings separately, to see whether the same factors applied in each kind of situation.

To make a long story short, we divided the respondents into five categories depending not on our judgment but on differences which appeared in a variety of their answers. For example, Ph.D.'s were noticeably different in many respects from non-Ph.D.'s. They expected from the organization and they got, different treatment from the bachelor's degree men. Individuals working in a laboratory where management valued contributions to knowledge were in a different atmosphere from those working in labs where the rewards went to useful products. Hence it was important to distinguish science or research-oriented laboratories from product-oriented or development laboratories.

It turned out that our university scientists all fell, as you might expect, into the research-oriented type of laboratory. This was not because we arbitrarily put them there, but their responses fitted that pattern. It turned out that our industrial scientists all fell into the development-oriented type of laboratory. We tried to get into some research- or phenomena-oriented industrial laboratories, but couldn't. In government, however, we had both basic or phenomena-oriented laboratories and product development-oriented laboratories.

Consequently we did separate analyses for Ph.D.'s in development labs, Ph.D.'s in research labs, non-Ph.D.'s in research, and non-Ph.D.'s in development labs.

Now let's return to the question of concentration versus diversity in the kind of work done. We analyzed the data by asking whether the man spent as much as five percent of his time on any of one of the five kinds of R&D functions. We simply tabulated for each individual the number of these different functions -- basic

research, applied research, product invention, product improvement, and technical services -- on which he spent at least a little time; and then examined how the number of his activities related to his technical performance. We found that Ph.D.'s in developmental laboratories did their best work if they performed four of these functions. Actually they were the most useful to the organization if they performed all five; but even scientifically they did better by not concentrating on research alone but doing something in product applications or technical services.

The engineers -- I use the term "engineers" loosely here, meaning non-Ph.D.'s working in a development-oriented laboratory, approximately half of whom did have engineering degrees -- earned their highest ratings both technically and in usefulness to the organization if they performed all five functions, that is, if they spent at least five percent of their time doing all five kinds of R&D activities.

What about the Ph.D.'s in a research lab? You might guess that they should be more specialized, concentrating possibly on one or two activities. But they also did their best work when they had four functions. Now these included research Ph.D.'s in the university; the best ones admitted spending at least a fraction of their time on practical applications.

In other words, the more effective scientists and engineers, when viewed either in terms of contribution to knowledge or to practical technology, did better, not when they concentrated on the world of science, and not when they concentrated on application, but when they kept one finger in several of these tasks. This suggests a kind of creative tension, I think.

Independence versus interaction

There was another paradox which appeared fairly commonly. Scientists usually assert -- at least the better of them do -- a desire for freedom to follow their own ideas. In our study, this was one of the strongest needs expressed in the interviews. How then did the personal need for freedom relate to performance?

Now to desire freedom is not the same as having it. As I mentioned earlier, the best scientists in the Ph.D. category were not the ones who were completely self-determining; the more productive ones had perhaps half the weight in deciding their own technical objectives. On the other hand, it is safe to say that whether or not they exercise complete self-determination, the better scientists were the ones who wanted freedom, and this came out in several ways. They relied on their own curiosity as sources of ideas; they relied on their own previous work as a stimulus in planning their future work; and they denied that their supervisors

were particularly valuable as a source of ideas. The ones who acknowledged that their boss might have some smart ideas turned out in fact to be relatively low producing. The high producing people, in short, the most effective scientists, were fiercely independent and self-reliant.

That is one side of the paradox. The other side appeared when we asked: "How many people do you talk with closely about your work?" We asked several questions on this -- "within your own section or team," "outside your team but in other parts of the organization," "or outside the organization." We also asked: "Among the five people you communicate with most, how often do you see them? -- everyday, twice a week, once a week, once a month?" On all of these measures of communication, we found that the more effective scientists in general communicated more often and more vigorously. They saw more people, and they were in touch with a wider circle.

Thus although our effective scientists were fiercely independent, they did not use this independence to withdraw. They reached out and communicated. They were in touch, and I think this fact jibes with a number of points that previous speakers have made. As laboratory directors they make it a practice to encourage communication, to encourage technical men to talk with other technical men -- both in the universities and in the engineering side of things.

This point was made even more sharply in a planning discussion a month or so ago. Dr. Hollingsworth for example, would ask his men: "When have you talked to lately?", and if he found a man who had not spoken recently with anyone outside of his own specialized group -- if he had not interacted with someone in the engineering products laboratory, or with a colleague in another institution -- he would get worried. From my results, rightly so.

Here you have a tension between intellectual independence and self-reliance, on the one hand, and on the other a willingness to go out and interact with other people. This situation recalled a passage which once struck me in high school when we were studying Emerson's essays. He said: "It is easy in the world to live after the world's opinion; it is easy in solitude to live after our own; but the great man is he who in the midst of the crowd keeps with perfect sweetness the independence of solitude." A creative scientist, like Emerson's great man, does not retreat into solitude nor does he allow his ego to be submerged in the world of men, but in the midst of the latter, he maintains his own independence.

This is not an easy thing to do. It seems to me, therefore, that one of the important functions of the management of research -- and I believe this applies to basic research as well as to development -- is to see that both polarities are encouraged and maintained. The question of self-reliance is often not easy for a technical man. He may be working in a field where no one knows what he is doing, and he can get discouraged. As previous speakers have mentioned, an important function of the supervisor is to be enthusiastic about what the individual has done, in order to build his self-confidence. Now perhaps many good scientists bring this quality of self-confidence with them. But you may also have on your staff able people who are intellectually on a par with the self-starting group, but for some reason don't have the nerve, as it were, to continue pursuing their own ideas in the face of indifference from other people. They need to be encouraged.

One of the best ways of encouraging a young scientist I suspect, is to see to it that he produces something worthwhile. You can either ask your scientists to make reports or you can eliminate this requirement; my personal feeling is that if a scientist has done something valuable, he should be asked to talk about it. It should not be left up to him to volunteer to report it or not, as he feels inclined. To encourage the person to write about or talk about what he has done, is a way of building his confidence and gaining the recognition of others.

Summary: creative tensions

Let me summarize the gist of these findings in the following way. It has seemed to me that many of our results point toward two general characteristics or features. On the one hand, an effective scientist needs to have some degree of protection from his environment; he needs to be sheltered in from pointless demands. Such protection can be called a factor of security. Autonomy is one example; to give an individual the right to plan his own work is to protect him from arbitrary demands of his environment. Another source is giving him some voice or influence in decisions affecting him; previous speakers have described mechanisms whereby the technical man is involved in decisions concerning him.

Specialization is another source of security. When an individual has a field of expertise about which he knows more than anyone else in the organization, he is assured freedom from arbitrary interruption. The possession of a Ph.D. serves the same function. Once a man has a Ph.D. he can wrap it around himself and say: "Don't bother me"; whereas the non-doctoral engineer doesn't have that protection.

(As an illustration, when we looked at the age of groups, the length of time that individuals had belonged to their sections, we found that the older groups were more likely to have a large proportion of Ph.D.'s; the newer groups were likely to consist of non-Ph.D.'s. Once you put a group of Ph.D.'s together, you let them stay, whereas with a group of non-Ph.D.'s you feel a need to re-organize them. The doctoral degree offers protection, against arbitrary management action.)

But our findings also showed the importance of another general factor. Effective scientists and engineers had some source of security, but they were not over-protected. They were exposed to some degree of challenge from their environment. One example was frequent communication with colleagues. Another evidence was that decisions affecting a man's work were not made by one or two people; the more different sources or positions in the organization were involved in deciding what a man did, up to a point, the more effective he was. The Ph.D. in a developmental laboratory who made his own decisions was not particularly effective either by scientific standards or by the company's standards. The Ph.D. who shared the decisions with one other source, such as his supervisor or a colleague, was somewhat more effective. The best situation was when four different sources, including top level management and clients, all had some degree of choice in deciding his assignments. Then his performance was at a maximum.

In a number of ways, then, we found that the best work came when scientists on the one hand had some source of security or protection, but on the other hand had some source of challenge or exposure to external demands. It was not a question of one or the other, nor of a halfway compromise between them, but the presence of both.

We can consider a new version of an old epigram, "Necessity is the mother of invention." Necessity is one source of challenge -- being faced with a problem which simply shouts for solution. I would rather call necessity or challenge, however, the "father" of invention instead of its mother, since necessity is a masculine quality. Security is the mother of invention. And when these two pressures or qualities come together -- security and challenge, feminine and masculine -- a creative tension is present which can give birth to technical achievement.

Questions and Answers following Dr. Pelz' Talk

Question from Audience

With respect to the categories that a scientist is involved in, I have found that a man whom I think very highly of as a colleague is usually also involved in symphony orchestras, or something else. Scientists whom you would rate highly from the point of view of being good at their science, usually have much more activity than just their science. They are usually involved in the community too. Have you found this at all?

Pelz

We did not collect data on that point, although I would tend to feel you are right. This leads to a question which always comes up within half an hour, so I will raise the point. Which is the chicken, so to speak, and which is the egg? If we find that effective scientists are more diversified in their interests, which comes first -- diversity or effectiveness?

Studies of creative individuals, whether in scientific or artistic areas, have found the following: if you present these individuals with patterns of lines, one pattern being confused, complex, or chaotic, the second pattern being neat, orderly well-structured, etc., and ask them which set of lines they like better, the more creative scientists will prefer the more complex and chaotic pattern. A simple problem does not challenge a creative person. Perhaps this is a manifestation of "curiosity." In the personality of these individuals is a "comfort with disorder" -- the ability to tolerate complexity, and perhaps a desire to master it.

Now a further question arises -- suppose that a certain scientist is bright (and I think there has to be a high level of what we call intellectual ability), but he lacks this personality characteristic of curiosity or interest in complexity. If you expose him to complex stimuli, can you also increase his scientific performance? I do not have definite data on this, but I tend to be an optimist; I feel that if you can induce an individual to interest himself in a broader range of problems than he normally would, out of this broader exposure will come the stimulation for more creative performance.

Question from Audience

I wonder, Dr. Pelz, if you would comment on the usefulness of this index of citations that Dr. Hollingsworth mentioned earlier -- whether this covers the social sciences, or not. I am not very familiar with this, and I notice that you did not use it in your studies. I wonder how useful it is, for instance in economics?

Pelz

My feeling is that it would be useful. A few studies suggest that the most outstanding figures in psychology, for example -- the ones judged to have a high quality of contribution -- have also been prolific producers. They have done a heck of a lot and published voluminously. Now the converse doesn't necessarily apply, but I suspect that to earn a title as an outstanding contributor one must have certain quantity of output. I would like to think a citation index would be a reasonably good approximation to such a measure.

There are other ways of evaluating performance. In one study that a colleague of mine did on medical sociologists (this field deals with sociological factors affecting the onset or spread of disease), he asked each of 200 people heading such projects to submit a recent report of work done, and had these reports evaluated by panels of outstanding people in the field. Perhaps judgments of that sort would be the thing I would trust most. But that is a difficult and time consuming operation. I suspect, although I have no evidence, that the results of such a process would correlate reasonably well with a citation index.

Question from Audience

Dr. Pelz, in your study did you note any apparent differences in basic research, say, in a university laboratory and basic research in places outside universities?

Pelz

I did notice one curious thing. We became interested in the question of age of the individual, and found that generally speaking above age 45 there was some decline in performance. It turned out to be more complex than that statement would suggest, but I will not go into the complexities. There was in general a decline, and the question arose -- did this happen because good people were promoted out of research altogether as they got older? I said, let's look at the university people only, because in the universities the best men would not be promoted out of the department. (They might get a deanship, but the number of deanships is relatively rare.) We then compared the basic research Ph.D.'s in government with those in the university, and found that in later years the university people recovered to a higher level than did research Ph.D.'s in government laboratories. We did a similar analysis in development laboratories -- Ph.D.'s in government versus industry, and found a similar trend. In industry the older scientists tended to achieve again in their later years, whereas the older government scientists did not return to the same level. Thus both in development and in research, productivity of the older government scientists tended to decline more sharply with age.

Vollmer

It is very difficult to summarize all of the important things that were said here in the discussion today, but I can think of three key words which are related to planning this kind of research activity. They have come up time and time again this afternoon. The first word is "people". It is important what kind of people you get -- this has to be part of planning. "Organization" is another key word -- it is important how you relate these people together to facilitate the right kind of collaboration and communication. The third word is something that is more than organization, it is "climate" -- a word that has been used quite a bit here. You have to plan for a climate in which phenomena-oriented research is born, grows, and flourishes in a productive way. A balanced emphasis upon security and upon challenge is an important part of this climate.

So we have to think in terms of a biological image of planning a phenomena-oriented research activity. This is not something that can be designed mechanically. We must leave you now to ponder what all this might mean in the context of your own organization.